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E. J. STONE, M.A., F.R.S., President, in the Chair.

H. J. Chaney, Warden of Standards, Board of Trade, 29 Chalcot Crescent, Regent's Park Road, N.W.;
B. J. Hopkins, 23 Weymouth Terrace, Hackney Road, E.;
Rev. T. Harley, 24 Amwell Street, E.C.;

were balloted for and duly elected Fellows of the Society.

Note on some Criticisms made by Mr. Stone on the Methods available for Determining the Solar Parallax. By David Gill, LL.D., Her Majesty's Astronomer at the Cape of Good Hope.

In the accounts which have appeared in the *Observatory* and the *Astronomical Register* of the interesting discussion which followed Professor Newcomb's remarks at the January meeting of the Society, there are some points raised which appear to deserve further notice.

I refer in particular to the criticisms of the President (Mr. Stone) on the Heliometric method of determining the Solar Parallax. If Mr. Stone is correctly reported, he gives the following criticism of the method:

"When the distance of a planet from two stars, one of greater and the other of less right ascension, is measured E. and W. of the meridian, by comparing the distance of each star from the planet as measured on the two sides of the meridian, we can get a determination of the parallax affected by no systematic errors but corrections for refraction, motion of planet in the interval, and small error due to error of scale over the change of distance measured. The result should, therefore, be an accurate one. But by taking each star we ought to get independent

determinations; and if these agreed very closely, it would prove that any systematic errors must have been reduced to very small limits. When, however, we have made the results from the two stars agree by the supposition of a change of scale value, although the result deduced may be true, yet its truth is not apparent from the equations; the check is lost."

I may ask, in the first place, whether it is fair criticism to condemn a method because the critic supposes it will not bear a test which it was never contemplated to apply to it, which would give no additional weight in the elimination of systematic errors if it were applied, and which would involve a mode of discussion that is obsolete and unscientific.

Let it be supposed that I had applied to my observations of *Mars* the test proposed by Mr. Stone; that I had deduced from measures of the stars preceding and following the planet precisely the same parallax, what additional value would the final result have had? I should simply have proved that, on the whole, a correct value of the temperature coefficient of the Heliometer had been employed. The result would not have proved that the light of the planet was differently influenced by refraction from that of a star, nor would any error have been eliminated that is not eliminated in the method of reduction which I adopted. It is besides infinitely more scientific and exact to proceed by a method of reduction which involves no assumptions as to temperature coefficients, or absolute scale values, but which in itself contains all the data for the elimination of errors that may exist in the determination of these quantities, which, after all, cannot be termed *constants*, for reasons given in my Memoir on the *Mars* Observations (p. 56).

It is perhaps desirable to recall the mistakes of Wichmann in his discussion of the parallax of the Star 1830 Groombridge. These mistakes have been most ably pointed out by Döllen (*Bull. Phys. Math. Acad. St. Petersburg*, t. xiii.) to be due to the adoption of the absolute method advocated by Mr. Stone; and he also shows the truthful conclusions that may be drawn from the same observations by a method similar with that which I have followed. Wichmann himself afterwards frankly acknowledged his error.

It is, in fact, a principle that is now accepted by all refined and accurate observers, that in delicate researches the observations must be so arranged that the possible errors shall take opposite signs, and, as far as possible, eliminate each other.

The point in Mr. Stone's criticism which most surprises me is his statement that the truth of the result for the class of errors taken account of by the equations (for Mr. Stone only discusses these) is not apparent from the equations.

I took especial pains (pp. 116–125 of my *Mars* Memoir) to explain the formation of these equations, and the question of their rigid consideration of all the errors named by Mr. Stone is not a matter of opinion but of geometry. I would be greatly

indebted to Mr. Stone if he would point out where these equations are not geometrically true, at least far within all practical limits.*

Mr. Stone goes on to say, "The method in one case is a very strong one indeed; but in the case of the necessity of supposing different scale values E. and W. of the Meridian, I must confess that I consider its strength problematical."

It seems to have escaped the notice of Mr. Stone that I have anticipated this objection, and at pp. 151–155 of my *Mars* Memoir I have given the results of a reduction in which the scale values E. and W. of the Meridian *are not supposed different but identical.*

The result of this reduction is in precise agreement with the more refined original reduction, and the close agreement of the separate results proves that the systematic differences of scale value E. and W. of the Meridian are confined within very narrow limits.

My contention, however, is that no matter how large the systematic errors of scale value produced by difference of temperature in the evening and morning observations may be, the final results will be entirely independent of such errors if reduced by the method employed in my original reduction. It is surely wiser and safer to assume that such errors may exist, and to employ a method of reduction by which they will be eliminated, than to adopt a crude and exploded method of discussion which ignores the existence of the carefully arranged means of

* There is only one assumption in the formation of the equations which is not rigidly exact—it is, that the relative positions of the comparison stars are assumed known. The effect of this assumption is that the accuracy of the observed change of the planet's R.A. between the evening and morning observations depends to a small extent on the tabular difference of R.A. of the stars of comparison. (I confine myself, for sake of simplicity, to the consideration of one coordinate.) An error in this difference will affect the measured change of the planet's R.A. by the quantity

$$\text{Error of } \Delta\alpha \text{ of comparison stars} \times \frac{\text{observed motion}}{\Delta\alpha \text{ of comparison stars}}.$$

About opposition the greatest change of R.A. between the observations of the evening and following morning appears to be about 300″ of arc, and the comparison stars may be taken as 2° apart, that is, each 1° distant from the planet on opposite sides of it. If we suppose the probable error of the $\Delta\alpha$ of the two comparison stars to be ±0″·25 (which is a large estimate considering the number and excellence of the meridian observations), the effect of this error on the measured change in the planet's R.A. would be

$$\pm 0''{\cdot}25 \times \frac{300}{7200} = \pm 0''{\cdot}01,$$

which would produce an error of less than 0″·003 in the resulting solar parallax! When it is further considered that the final tabular motion of the planet is derived from the adopted star places, it is obvious that not the remotest systematic error can remain from the assumption in question.

eliminating possible errors, and affords no measure of the real accuracy of the final result.

It is of course open for anyone to assert dogmatically that the only crucial check is to determine the parallax separately from the absolute distances measured from stars preceding and following the planet. But let us take a well-known and strictly parallel case to illustrate the fallacy of such an assertion. It is now pretty generally admitted that for field operations (that is, when star places are assumed known) the most accurate method of determining latitude is that known as Talcott's method. This consists in measuring the *difference* of zenith distance of two stars culminating at nearly equal and opposite zenith distances from the place of observation.

A refined observer who desired to determine his latitude with the greatest precision would also be careful to employ as many pairs of stars in which the Z.D. of the North Star exceeded that of the South Star, as pairs in which the Z.D. of the South Star exceeded that of the North Star.

The observer would then be in a position

1. To discuss the resulting latitude, supposing the screw value unknown, and to determine the screw value from the observations;

2. To trust to the general symmetry of the arrangement of the comparison stars for the elimination of any error in the adopted screw value;

3. To deduce the latitude from absolute zenith distances of the stars (which we shall suppose also to have been measured), and to form our estimate of the accuracy of the resulting latitude from the agreement of the absolute results from the North and South Stars separately.

The first of these methods of reduction corresponds with that employed in my original reduction of the *Mars* stars.

The next represents the second discussion to which I have referred, in which the scale value is supposed known, and not to change between the East and West observations.

The third represents the method advocated by Mr. Stone.

If Mr. Stone is consistent he is no less bound to maintain that the check is lost in Talcott's method because the latitude is deduced from the *differences* of the opposite zenith distances, and not from the *absolute* opposite zenith distances; and that the accuracy of the resulting latitude would be more fairly represented by the agreement of the latitudes deduced separately from the absolute altitudes observed N. and S. of the zenith, than from the difference of Z.D. of pairs of opposite stars. He would, in fact, ignore the powerful elimination of errors of flexure and refraction secured by Talcott's method, in the same way that he would ignore the elimination of error of scale value and refraction in the Heliometer method which I have advocated.

If, in fact, Mr. Stone insists on the absolute method in the case of the Heliometer, as opposed to the elegant method of dif-

ference, I do not see how he can avoid insisting on it in case of the Talcott latitude method; and if he does so, it is safe to assert that his opinion will not be seconded by that of any competent living astronomer who has ever made a refined investigation with one instrument or the other.

Mr. Stone's criticism generally does not, however, touch the real weak point of my result obtained from the *Mars* observations. That point is the possibility, and even probability, that the average light of *Mars* is of different refrangibility from that of the average light of the comparison stars, and that on account of the ruddy light of *Mars* it is not impossible that my value of the solar parallax is slightly too great.

Possibly partly to this account, partly to chromatic dispersion, is due the large value of the solar parallax that has been obtained from meridian observations of *Mars*. We have no data by which to estimate how far it is possible there should exist, besides, a systematic error in measuring zenith distances, having the same sign for nearly all observers. There are not wanting instances of similar nearly universal errors—errors whose common sign may be attributed to the fact that all the observers are human beings more or less similarly constituted.

Thus in determining the personal equation of the Cape observers for stars of different declination, I found that there was a marked difference in the relative equations on opposite sides of the zenith. Accordingly, a number of zenith stars were observed in which each star was observed over the first two or three wires, with the observer's feet to the South, and over the last two or three wires with his feet to the North. In observing the next star the order was reversed, in order to eliminate errors of the wire intervals.

The following are the results so obtained; each result depends on the observations of a different night:—

	s	No. of Stars.	Mean. s
Finlay	$= +0^{s}\cdot063 \pm \cdot017$	21	
	$+ \cdot079 \pm \cdot011$	32	$+0^{s}\cdot079$
	$+ \cdot111 \pm \cdot025$	16	
	$+ \cdot078 \pm \cdot012$	28	
Maclear	$= +0^{s}\cdot145 \pm \cdot025$	23	
	$+ \cdot117 \pm \cdot017$	23	$+0^{s}\cdot122$
	$+ \cdot112 \pm \cdot023$	20	
	$+ \cdot115 \pm \cdot019$	24	
Pett	$= +0^{s}\cdot056 \pm \cdot020$	18	
	$+ \cdot009 \pm \cdot019$	16	$+0^{s}\cdot015$
	$- \cdot023 \pm \cdot021$	22	
	$+ \cdot016 \pm \cdot018$	24	

	s	No. of Stars.	Mean. s
Freeman =	+0·014 ± ·030	24	
	+ ·076 ± ·018	11	+0·066
	+ ·144 ± ·027	23	
	+ ·029 ± ·022	17	

These results are taken out in the sense: clock slow, observer facing N.; clock slow, observer facing S.*

Precisely similar results were afterwards obtained by the use of a reversing prism attached to the eyepiece, by turning which through 90° the star could be made to appear to move either from right to left or left to right.

Here, then, is a very marked error, having the same sign, common to all our four experienced observers.

Again, in the case of the meridian observations of the *Mars* stars, it was found that nearly all observers made the Right Ascension of the final stars too great.

Is it not possible, in face of these facts, that in observing zenith distances of a large and unsymmetrically-coloured disc like *Mars*, when viewed at a considerable zenith distance, there may be a very sensible systematic error, having the same sign for nearly all observers?

We have at least no proof to the contrary, and my friend Dr. Elkin has suggested a very admirable test. If *Mars* were observed at Northern and Southern observatories at an opposition when the planet is nearly as distant as possible from the Earth, and if such a systematic error as I have suggested really existed, then the parallax resulting from such observations should be still larger than that derived from the oppositions when *Mars* is near the Earth.

There are excellent opportunities for making such determinations in 1884 and 1886.

There is, however, one method of determining the solar parallax which I believe to be free from all systematic error. I refer to the method which I followed in the case of *Juno*. These observations were confessedly incomplete, and afford, in themselves, no crucial test of the method.

There is a test, however, which fortunately depends on no theoretical considerations, to which I believe no exception can be taken, and which is capable of being easily applied.

Anyone who has taken part in the recent measures of *Victoria* and *Sappho* will admit that it is impossible to distinguish between the appearance of either of these planets and that of a fixed star of similar brightness. It is, therefore, possible to test the method by employing a fixed star instead of a planet. We

* These results may help to explain the systematic discordances which Dr. de Ball has found between the Right Ascensions of the Cape and Northern Catalogues.

know that a fixed star has no sensible diurnal parallax, and therefore we may measure with the Heliometer its distance from two or more fixed stars, and determine whether there is any apparent change in the relative position of this star with respect to its neighbours, as the result of a series of measures made both East and West of the Meridian.

As an example of such a test, I give the results of my last measures of the kind.

The circumstances are far from the most favourable possible. The central star (*Sirius*) is not in the line between the comparison stars *a* and *b*, the angle at *Sirius* being 165°. Also East of the Meridian the stars are nearly horizontal, and West of the Meridian they are nearly vertical.

The circumstances of definition were not favourable, the images and steadiness being 2–3 (where 1 is the most favourable, and 3–4 the worst in which such measures can be attempted). The light of the bright star was reduced by wire gauze screens which make the image of *Sirius*, as seen in the Heliometer, quite similar in brightness and appearance to the 7th magnitude stars of comparison.

1882, *December* 24.

Evening Observations.

S. Time.	Sirius and *a*	Refrac.		Sirius and *b*	Refrac.	
h m	″	″	″	h m	″	″	″
2 38·2	3683·854	1·287	3685·14	2 46·9	3627·088	1·029	3628·12
3 15·6	84·497	1·184	·68	3 0·2	26·907	1·016	27·92
3 23·6	83·995	1·145	·14	3 32·7	27·075	1·016	28·09
3 57·2	84·240	1·094	·33	3 38·8	27·062	1·029	28·09

Morning Observations.

9 18·8	3683·069	1·763	3684·83	9 28·8	3626·200	1·673	3627·87
9 55·2	82·721	2·200	84·92	9 46·7	·071	1·853	27·92
10 3·2	83·172	2·354	85·53	10 13·4	25·556	2·213	27·77
10 32·4	82·155	3·088	85·24	10 22·1	25·724	2·380	28·10

These observations give in the mean

	Sirius and *a*		Sirius and *b*		*a*–*b*
	h m	″	h m	″	″
Evening observations	3 18·7	3685·32	3 14·7	3628·05	57·27
Morning „	9 57·4	·13	9 57·8	27·93	57·20

Each separate result is the mean of two bisections, one in each position of the segments with respect to the zero point.

The four results for each star on each occasion form a com-

plete set in which every variation of the possible methods of bisection is represented, both as to the final direction of the motion by which the bisection is completed and with reversal of the Heliometer on its axis of rotation. These conditions are of course unfavourable to the apparent agreement of the separate results, but very favourable to the accuracy of the mean.

The very exact agreement in the value of $a-b$ from the evening and morning observations is of course, to some extent, a matter of chance. From the discussion of a very large number of similar observations, which will soon be ready for the press, the probable error of the difference $(a-b)$ comes out $\pm$ 0″·15 for eight bisections of each star, made in the order and with the precautions above described, and the same observations agree in showing that the resulting measure of $a-b$ is quite the same, whether measured East or West of the Meridian.

Now, if instead of *Sirius* we had a minor planet, and if a station were occupied near the Equator, and comparison stars employed of nearly the same declination with the planet, and on opposite sides of it, and the declination were small, this distance $a-b$ would represent twice the parallax due to the altitude. That is, the difference between $a-b$ in the evening and morning after allowing for the planet's motion would be *four* times the parallax at the adopted altitude of observation.

Put this altitude even so high as 45°, and we get for the probable error of the solar parallax from one night's observation

$$\pm 0''{\cdot}15\sqrt{2} \times \frac{\Delta\sqrt{2}}{4} = \pm 0''{\cdot}15\Delta,$$

where Δ is the planet's distance from the Earth, that of the Earth from the Sun being unity.

If planets as favourable as *Victoria* and *Sappho* were observed, this would reduce the probable error of the determination of the solar parallax to

$$\pm 0''{\cdot}12.$$

as the result of a single night's measures.

The method is not liable to the smallest uncertainty, if the instrument and observer have been first proved by the tests I have indicated. It is certain that from the observation of the oppositions of two or three minor planets by this plan an entirely reliable value of the solar parallax could be obtained, and that at a cost of one-fourth of the last British Transit of *Venus* rate, including the cost of a new Heliometer, which would afterwards be available for other researches.

I have added the latter part of these notes in answer to Mr. Stone's expression of his opinion "that none of our methods (for determining the solar parallax) can be considered as free from the

suspicion of systematic errors." It would be of great interest if Mr. Stone would point out his objections to the method I have described and to the tests I have proposed.

Royal Observatory,
Cape of Good Hope:
1883, *March* 6.

On the Position of λ Ursæ Minoris. By John Tatlock, Jun.

(*Communicated by Prof. T. H. Safford.*)

The present paper is intended as a continuation of the paper published under the same title by Prof. T. H. Safford in the *Monthly Notices* for June 1878.

This paper takes up the subject where Prof. Safford left off, and has been prepared, under his direction, according to the method and formulæ given by him in Vol. IV., Part 1, of the Annals of the Observatory of Harvard College, where he has given a similar discussion of this star, based on the data accessible in 1862.

The method given in the foregoing publication, and used in this discussion, is as follows: Let η be the correction to the assumed A.R. for 1855·0; η' the correction for n years to the assumed proper motion in A.R.; i the correction to the assumed Dec. for 1855·0; and i' the correction for n years to the assumed proper motion in Dec. In this discussion we have assumed $n=30$.

Also let a' and δ' be the star's A.R. and Dec. for the time $t+1855$, referred to the coordinate planes of 1855, δ the star's Dec. at the time t referred to the planes of 1855, ξ the angle at the star, used in referring the proper motion for 1855 to the planes of $t+1855$, and ω and ω' the weight in A.R. and Dec. respectively. Now making

$$\frac{\eta}{15}=w,\ \frac{\eta'}{15}=x,\ i=y,\ i'=z,$$

and also in A.R.,

$$a=\sqrt{\omega}\ 15\cos\delta\cos\xi$$

$$b=a\ \frac{t-1855}{30}$$

$$c=\sqrt{\omega}\sin\xi$$

$$d=c\ \frac{t-1855}{30}$$

$$v=\sqrt{\omega}\ \Delta a'\cos\delta',$$

taking $\Delta\alpha'$ in the sense $c-o$, and in Dec.,

$$a = -\sqrt{\omega'}\ 15 \cos\delta \sin\xi$$
$$b = a\,\frac{t-1855}{30}$$
$$c = \sqrt{\omega}\cos\xi$$
$$d = c\,\frac{t-1855}{30}$$
$$v = \sqrt{\omega'}\,\Delta\delta'\ (c-o),$$

we shall have the form of the equation of condition as follows:—

$$0 = v + aw + bx + cy + dz$$

In Prof. Safford's paper in the *Monthly Notices* he has marked some of the values of $\Delta\alpha' \cos\delta'$ and $\Delta\delta'$ as derived at second-hand from the investigations of others. In the present paper these values have been verified by reference to the authorities themselves. In one or two cases the authorities cited in Prof. Safford's paper have been replaced by a newer or more complete discussion. For instance, Auwers's reduction of Bradley has been used instead of the value given in Prof. Safford's paper, and Rogers 1875 has been substituted for Rogers 1872, as it includes the latter.

The "observed" positions have sometimes been formed by the combination of the results for two or more years, notably in the case of the Greenwich and Radcliffe Catalogues. In all such cases the proper motion from the epoch of the Catalogue place to the epoch of the "observed" place has been applied.

The following table gives the values of $\Delta\alpha' \cos\delta'$ and $\Delta\delta'$, which have been used, in addition to the values given in Prof. Safford's paper:—

Catalogue.	$\Delta\alpha' \cos\delta'$	$\Delta\delta'$	Weights
Bradley 1755	+0″·09	−1″·26	1, 1
Struve 1815	−0·15		1
Schwerd 1828	+0·16	+0·02	2, 2
Radcliffe 1860	−0·21	+0·62	1, 1
Radcliffe 1867	−0·64	+0·29	½, 4
Rogers 1875	+0·15	−0·35	3, 2
Airy 1878	+0·06	−0·12	3, 4
Airy 1880	+0·22	−0·04	3, 4

Treating the resulting equations of condition, 56 in number, by the method of least squares, we find the following system of normal equations:—

$$6{\cdot}5768w - 0{\cdot}9431x - 0{\cdot}0050y + 0{\cdot}2427z + 0{\cdot}2280 = 0$$
$$-0{\cdot}9431w + 3{\cdot}5262x + 0{\cdot}2552y - 0{\cdot}0990z - 0{\cdot}4326 = 0$$
$$-0{\cdot}0050w + 0{\cdot}2552x + 89{\cdot}5932y - 6{\cdot}0503z + 8{\cdot}4199 = 0$$
$$0{\cdot}2427w - 0{\cdot}0990x - 6{\cdot}0503y + 37{\cdot}1541z - 1{\cdot}1937 = 0$$

The final equations are

$$w - 0{\cdot}1434x - 0{\cdot}0008y + 0{\cdot}0369z + 0{\cdot}0347 = 0$$
$$x + 0{\cdot}0750y - 0{\cdot}0189z - 0{\cdot}1179 = 0$$
$$y - 0{\cdot}0675z + 0{\cdot}0940 = 0$$
$$z - 0{\cdot}0175 = 0$$

The solution of which gives the following values of the unknown quantities.

$$w = -0{\cdot}017^{s}$$
$$x = +0{\cdot}124$$
$$y = -0{\cdot}083''$$
$$z = +0{\cdot}018$$

The assumed position for 1855·0 is $\alpha = 20^{h}\ 8^{m}\ 30^{s}{\cdot}200$ and $\delta = 88^{\circ}\ 52'\ 32''{\cdot}30$, and the values of the proper motion are $-0^{s}{\cdot}073$ and $+0''{\cdot}01$ in A.R. and Dec. respectively. Applying the corrections which we have found, the corrected places for 1855·0 are

$$\alpha = 20^{h}\ 8^{m}\ 30^{s}{\cdot}183$$
$$\delta = 88^{\circ}\ 52'\ 32''{\cdot}22$$

And the corrected values of the proper motion are, in A.R.,

$$-0{\cdot}069^{s};$$

in Dec.,

$$+0{\cdot}0106''$$

From these elements the position for 1885·0, without proper motion, is

$$\alpha = 19^{h}\ 38^{m}\ 57^{s}{\cdot}705$$
$$\delta = 88^{\circ}\ 57'\ 19''{\cdot}34$$

And the amount of proper motion is

$$\Delta\alpha = -2{\cdot}365^{s}$$
$$\Delta\delta = +0{\cdot}233''$$

Hence the position for 1885·0 is

$$\alpha = 19^{h}\ 38^{m}\ 55^{s}{\cdot}340$$
$$\delta = 88^{\circ}\ 57'\ 19''{\cdot}57$$

Dr. Auwers gives for the same epoch,

$$\alpha = 19^{h}\ 38^{m}\ 55^{s}.704$$
$$\delta = 88^{\circ}\ 57'\ 19''.46$$

Field Memorial Observatory, Williams College, Williamstown, Mass., U.S.A.:
1883, *Feb.* 21.

Measures of the Companion to Sirius. By S. W. Burnham.

My measures of the companion to *Sirius*, made on ten nights during the present season with the 18½-in. Refractor of the Dearborn Observatory, are as follows:—

1882·988	40°·6	9″·16
83·000	40·3	9·27
83·074	41·1	8·99
83·107	39·9	9·02
83·115	39·8	9·17
83·134	40·1	8·90
83·137	38·1	9·17
83·151	41·0	8·92
83·153	40·6	8·91
83·159	39·6	8·86
Mean = 1883·110	40·1	9·05

Chicago:
1883, *March* 30.

Observations of Comet c, 1881 (*Schäberle*), *made at the Cambridge Observatory with the Northumberland Equatorial and Square-bar Micrometer.*

(*Communicated by Prof. J. C. Adams, M.A., F.R.S.*)

Green. M.T.	Aberration Time.	App. R.A.	Parallax.	App. Decl.	Parallax.	Comp. Star.	No. of Comp.
1881.		h m s	s	° ′ ″	″		
Aug. 24·35221	−·00332	11 59 49·113	+0·782	+37 19 33·28	+10·67	*a*	3
24·37579	−·00332	12 0 12·808	0·752	37 14 52·32	11·49	*a*	5
26·34636	−·00335	12 30 21·228	0·729	30 38 14·45	10·58	*b*	7
26·36421	−·00336	12 30 36·054	0·720	30 34 25·38	11·12	*b*	6
,,	,,	36·027	,,	24·77		*c*	6
27·37251	−·00339	12 43 46·641	0·686	27 4 55·38	11·30	*d*	11

Assumed Mean Places for 1881·0 *of Stars compared with Comet* c, 1881 (*Schäberle*), *with Reductions to Apparent Places for the night of Observation.*

	Right Ascension.	Reduc.	Declination.	Reduc.	Authority.
	h m s	s	° ′ ″	″	h
a	12 0 36·610	+1·576	+37 26 53·40	−8·45	Bessel (W_2) 11–1170, 71
b	12 30 5·613	1·701	30 35 0·37	−7·73	„ 12–614, 15
c	12 27 36·592	1·699	30 41 48·71	−7·85	„ 12–554
d	12 43 58·904	1·770	27 4 51·51	−7·49	Cambridge Zones, April 30, May 11.

On Mr. Finlay's Pre-Perihelion Observations of the Great Comet (b) 1882.

(*Communicated by H.M. Astronomer at the Cape.*)

The following are the results of the observations obtained by Mr. Finlay on the morning on which he first saw the comet, and on the following morning. They are, I believe, the earliest exact observations of this remarkable body.

The observations were made with the filar micrometer of the 6-in. Equatorial. The results are corrected for refraction.

Cape M. T.	Right Ascension.	No. of Obs.	N.P.D.	No. of Obs.
d h m s	h m s		° ′ ″	
Sept. 7 17 23 15·7	9 31 44·54 − ·054*p*	14		
7 17 26 5·3			90 59 49·3 + ·551*p*	8
8 17 20 10·5	9 40 2·14 − ·054*p*	5		
8 17 27 16·7			90 56 22·8 + ·552*p*	5

The following are mean places of the stars of comparison, derived from recent observations with the Transit Circle :—

		R.A. 1882·0.	Corr.	N.P.D. 1882·0.	Corr.
	°	h m s	s	° ′ ″	″
For Sept. 7	Arg. −0·2229	9 32 11·78	+1·82	90 56 40·57	+10·71
8	„ −0·2256	9 44 23·16	+1·82	90 52 18·46	+11·13

The mean places are for 1882·0; the corrections are to the apparent place on dates of observation.

Note on the Nucleus of the Great Comet (b) 1882. By David Gill, LL.D., H.M. Astronomer at the Cape of Good Hope.

(*Extract from a letter to Mr. Knobel.*)

In reply to the question which you ask on behalf of the Society, viz. Whether before perihelion the Great Comet of 1882 showed a duplex or compound nucleus, the observations recorded

by Mr. Finlay and Dr. Elkin on September 7 and 8, and printed in the *Monthly Notices*, vol. xliii. p. 21, prove clearly that no duplicity could be detected with our optical means on these dates. Dr. Elkin describes the nucleus as a sharp, well-defined disk 10″ or 15″ in diameter and strongly condensed in the centre.

A short glimpse which I obtained between clouds on September 9 confirmed this view. The weather was unfavourable till the 17th (the day on which the disappearance of the comet at the Sun's limb was noted). The nucleus was certainly single on that day; half an hour before the disappearance of the comet it was measured by Mr. Finlay as 4″ in diameter.

The day following perihelion passage the comet was observed by myself with the Transit Circle. The nucleus was then undoubtedly single, and certainly rather under than over 4″ in diameter; in fact, as I have described it, it resembled very much a star of the 1st magnitude seen by daylight.

Mr. Finlay and Dr. Elkin made careful drawings on the 17th, and I made one on September 18, and we have a continuous series of drawings of the comet down to September 28, which prove that within the limits of our optical means the nucleus was single till that day inclusive.

The morning of September 29 was clouded.

On September 30 Mr. Finlay remarks in his note-book, "There seem to be two balls of light in the head." Dr. Elkin, with the Heliometer and low power, noted on the same morning that the nucleus was elongated.

On October 1 Mr. Finlay's sketch shows that the balls of light (as he calls them) were farther apart, but conected by a line of light.

On October 9 Mr. Finlay noted that there were several points of condensation in the nucleus; of these the two extreme points were brightest. These differed $1\frac{1}{2}$ second in R.A.

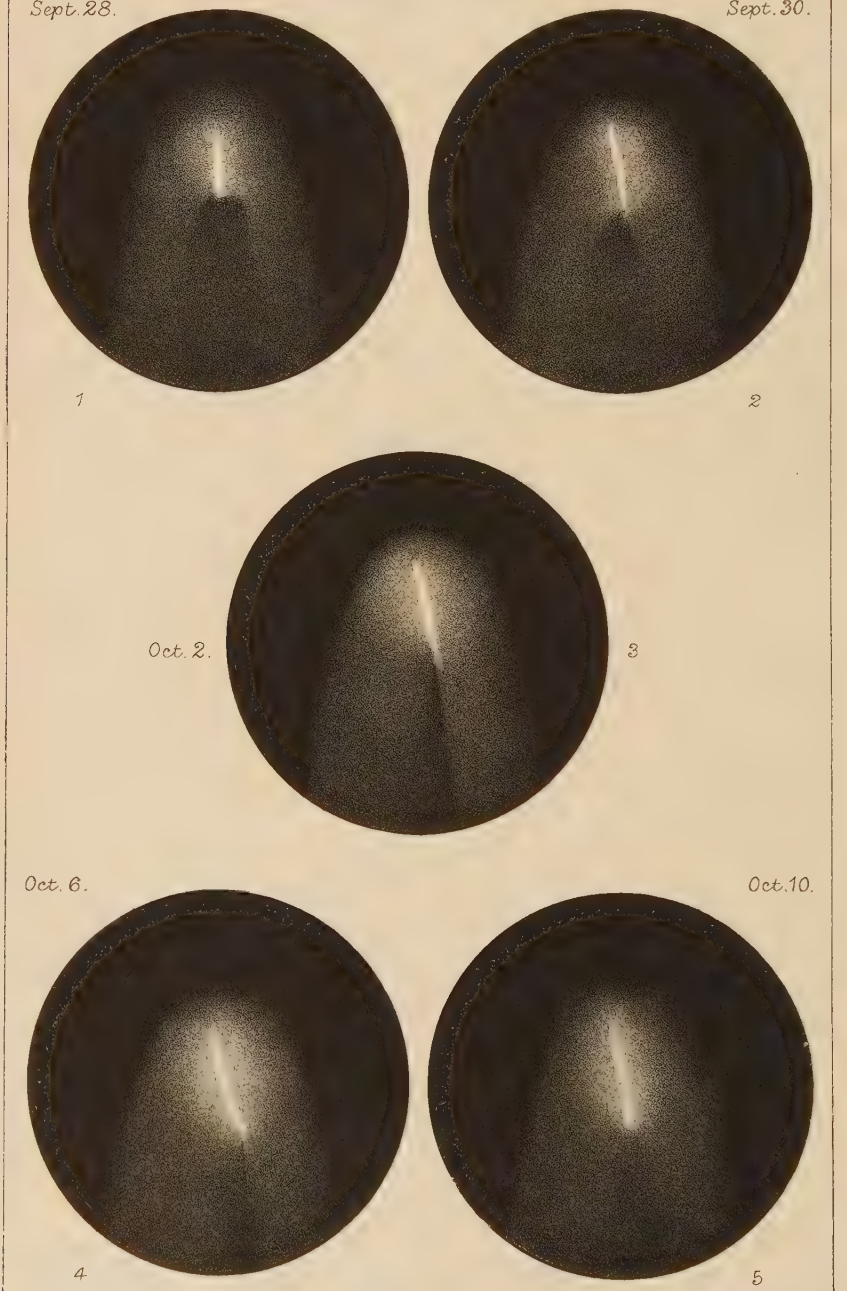
On October 10 several bright points were noted, the middle ones slightly south of the line joining the extreme ends.

On October 12 Mr. Finlay makes the line of nuclei 39″ in length.

On October 17 I counted 5 points of condensation in a pale cigar-shaped nucleus, which, in a popular paper communicated to the Cape *Quarterly Review* (a copy of which I send), I have described as a narrow line fully a minute of arc in length, with five little nuclei from 2″ to 3″ of arc in diameter, looking like very small beads strung on a thread of worsted. But to realise this description it is necessary to suppose the beads to have very large holes, and the worsted to be fluffy, so as to swell out and meet round the beads.

On November 1 Mr. Finlay found the nucleus (or line of condensation) to measure $3\frac{1}{2}$ seconds of time in R.A., and that it had become much more indistinct.

COMET *b* 1882.



W.H. Wesley lith.

West Newman & Co imp

Drawn by Major G.E. Strahan, R.E.
at Mussooree, India.

Last night (March 6), though the comet is a conspicuous object in a good opera-glass, the nucleus, or head, is faint in the extreme, most difficult to observe in R.A., and liable to large personal error in observing. It is an ellipse of which the major and minor axes are about $3\frac{1}{2}'$ and $1\frac{1}{2}'$ respectively. It is now diminishing in apparent size; it seems to have attained its maximum apparent size in December.

The places of the comet obtained here depend from September 30 to November 23 on measures made to the following extremity of the nucleus, because till the latter date there was a distinctly brighter condensation towards that point. Previous to November 23 observations were made both with the Heliometer and the Equatorial. After November 23 observations were confined to the Equatorial, and it was also found necessary to observe the centre instead of the extremity of the nucleus.

The necessary data for reduction to any desired point of the nucleus are, however, available. But until it is possible to publish the drawings of the nucleus I am convinced that no satisfactory account of the changes in the nucleus or discussion of the orbit of the comet can be made, and on this account I do not propose to publish the Cape observations until the necessary drawings are published at the same time.

Note on Sketches of the Great Comet (b), 1882. By Major G. Strahan, R.E.

I send herewith a few sketches of the comet of last September, with the hope that they may be of interest to the Fellows of the Society who may not have been favoured with such clear skies as I was at Mussooree (at an altitude of 6700 feet), in the North-West Provinces of India. I had intended making a much longer series of them, but the changes in the comet's appearance after Oct. 10 were so trifling as to make it hardly worth while. The instrument used was a Browning-With Reflector, with an aperture of $8\frac{3}{4}$ inches, and power of 160. Unfortunately I had no micrometer available; the dimensions are, therefore, merely estimations.

Calcutta:
1882, *Dec.* 1.

Note on Drawings of the Great Comet (b), 1882, *made at the Observatory, Arcetri, Florence.* By Wilhelm Tempel.

Continued ill-health and an almost uniformly cloudy sky greatly interfered with my observations of the comet of September, and I was able to make but few sketches of it.

The accompanying sheet of drawings was destined for the Royal Astronomical Society, but I have hesitated about sending it, as it is merely the work of my own hand, with the assistance of the telescopes of the Observatory here; and it would appear that of late much greater value is attached to photographic representations. I trust that the Society will kindly receive this work of mine, and not criticise it harshly.

As these sketches were made as faithfully as possible from nature, any explanation of them would be superfluous, for, as Father Secchi has well said, "Le figure dicono più che molte parole."

The drawing H, however, requires a short explanation, as it has no connection with the comet. It represents two fine threads of glass crossing the focus of an eyepiece. These threads are tubular, and present the appearance of two black lines separated by a single white one. In the figure H the two threads A and B differ in diameter, A $= \frac{3}{1000}$, B $= \frac{12}{1000}$ of an inch.

It is well known that most great comets show a dark line in the middle of the tail; this is seen most plainly in the axis of the tail near the head. The great comet of September also, in the beginning of October, clearly showed this dark line, and the second tail, pointing towards the Sun, had also the appearance of a tube—that is to say, the sides were bright and nebulous, while the axial line was dark.

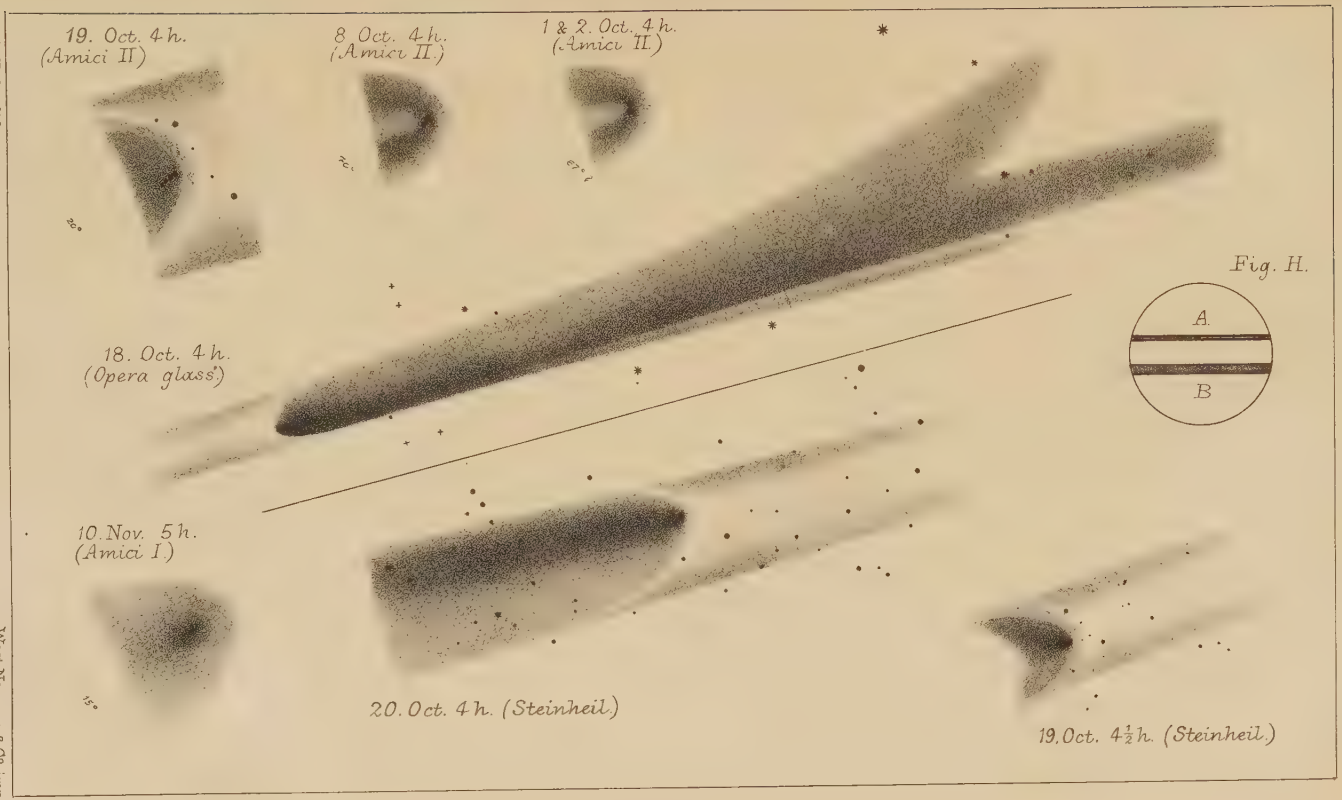
The appearance of the comet's tail presents an evident resemblance to these glass threads. Still, I offer no hypothesis, simply remarking that it is the duty of every observer to seek for familiar forms to compare with such as are difficult of comprehension.

The position-angle of the second nucleus of the comet with the axis of the tail, as given in the drawing, is only an estimation, not having been measured.

Arcetri, Florence:
1882, *December.*

Notes on the Great Comet (b) 1882. By E. E. Markwick.

I forward a condensed account of my observations of the Great Comet. I was only able to secure one observation of it before perihelion, and this was on September 14, at Durban. When first seen, about 5^h a.m., it was about 5° above the horizon, and it appeared ruddy, probably through haze. I looked at it



Drawings of the great Comet (b) 1882.
by Wilhelm Tempel.

through a small achromatic of $1\frac{3}{4}$-in. aperture, which showed the head of a woolly texture. The comet remained visible till nearly sunrise.

On September 18 I looked very carefully for it, but doubtless its proximity to the Sun prevented its being seen.

From the following date the observations were made at Pietermaritzburg with a $2\frac{3}{4}$-in. achromatic, powers 45 to 140.

September 25, $16^h\ 30^m$.—Saw the comet. It was a most magnificent object, with a superb tail many degrees in length. When first seen its head was below the horizon, and it presented an astonishing appearance, like a long white cloud on the horizon. It gradually rose and displayed itself as a lovely sight. The head was pointed, with a bright nucleus, the general tint a soft white. The tail lay nearly in the ecliptic, or parallel to it. The S. edge of the tail was sharply defined, the N. edge softened off. There was a dark streak or shading down the tail, rather towards the S. edge. In the telescope the nucleus was bright and star-like. Watched it till $17^h\ 30^m$, when the nucleus and a little of the head were still visible.

September 30, $16^h\ 45^m$.—Noted the tail as markedly curved. The comet was one of the most exquisite sights I have ever witnessed, seen in a very pure sky. The head was brilliant, and equal to a 1st magnitude star, but the light was more diffused than that of *Jupiter*. The glass did not reveal much, except that the nucleus was an elongated patch of light, the direction of its length not coinciding with the axis of the comet. The finest feature (only seen with the naked eye) was a delicate outlying border, looking like thin gauze or spray, which started back from the head and formed the borders of the tail for some way back. The effect of this was most beautiful.

October 1, between 16^h and 17^h.—Position of head estimated roughly at R.A. $10^h\ 46^m$; S. Dec. 8°; breadth of tail at widest part, $1\frac{3}{4}°$; length, $12\frac{1}{2}°$. Many stars seen through the tail, and these shone quite brightly. There was still the pronounced dark streak in the tail, and further I fancied I could see traces of other streaks. The same delicate halo surrounded the head and sides of the comet proper. In the telescope, by quickly moving it backwards and forwards, this delicate light could be seen in advance of the nucleus.

October 4, 16^h.—Tail appears a little longer, although possibly not quite so bright. The streak was barely visible; some hazy light round the head. The nucleus appeared woolly in the telescope, and the hazy envelope could be seen $1\frac{1}{2}°$ in advance of the nucleus. The tail, if produced, would pass almost centrally over *a Hydræ*. Position at $16^h\ 30^m$: R.A. $10^h\ 38^m$; S. Dec. 8° 48′; length of tail, 14°. This and the following positions were obtained by comparing comet with stars near its course, the *Uranometria Argentina* forming a most excellent and trustworthy guide, as it appears to represent the heavens with the utmost fidelity. South, preceding the comet's head, at this

time were seen, about $1\frac{1}{2}°$ distant, two wisps or pieces of nebulous-looking light. Whether they had anything to do with the comet I cannot say; but I can trace no nebula as being in this position, and moreover I have not been able since to recover them.

October 5, $15^h\ 30^m$.—General features of comet unchanged. No streak was seen, and the end of the tail seemed rather ragged. Direction of tail towards α *Hydræ*. Position at 16^h: R.A. $10^h\ 32^m\ 53^s$; S. Dec. 9° 24′.

October 9, $15^h\ 20^m$.—S. edge sharp. The end of the tail and its N. edge very much softened off; in fact the S. edge seemed to project at the end into a sort of wisp. Position at $15^h\ 30^m$: R.A. $10^h\ 27^m$; S. Dec. 11° 14′. I noticed that a star near λ *Hydræ*, over which the tail passed, was invisible to the naked eye. Nucleus elongated and inclined at an angle to axis of comet. Faint halo round head seen, with naked eye, quite 1° in advance of nucleus.

October 10.—Comet well seen in a very clear sky from $15^h\ 40^m$ to $16^h\ 20^m$. S. edge of tail just grazed λ *Hydræ*. Tail extended to beyond α *Hydræ*. Halo visible in advance of nucleus some 2°. Length of tail 18°, not including nebulosity in front of head. Position at 16^h: R.A. $10^h\ 21^m\ 24^s$; S. Dec. 11° 22′.

October 11, $15^h\ 30^m$.—λ *Hydræ* just within the tail, shining brightly—a very pretty sight. Comet white and silvery. One or two double stars in and near tail, a brilliant spectacle. Halo visible 2° in advance of nucleus; length of tail 17°. Position at $15^h\ 30^m$: R.A. $10^h\ 24^m\ 27^s$; S. Dec. 11° 45′.

October 15, $14^h\ 45^m$.—Very clear sky. Saw a bright red meteor near head. Comet's light a little fainter I think generally. Tail of a whitish, silvery light, very broad at end; a wisp projected at the end beyond the other part. Delicate halo round the head and streaming back by the sides. Position from three small stars near; at $15^h\ 15^m$: R.A. $10^h\ 17^m\ 49^s$; S. Dec. 13° 19′.

October 23, $15^h\ 45^m$ to $16^h\ 15^m$.—Light palpably fainter, yet comet is still a superb object, head being as bright as a 1st magnitude star. Nucleus rather better defined than before. It looks like a line of light, the same being inclined at an angle to the axis of the comet. There were a star and a double star a little way from the head, forming a beautiful sight. A faint telescopic star of about 9th magnitude close to nucleus n. f. Traces of dark streak down the tail. Position at 16^h: R.A. $10^h\ 7^m\ 28^s$; S. Dec. 16° 44′; length of tail 18°.

November 2, $13^h\ 30^m$.—Comet still well seen, although very much fainter; moreover, it was dimmed by an old moon not very far off. Position at $14^h\ 25^m$: R.A. $9^h\ 52^m\ 57^s$; S. Dec. 20° 9′; length of tail, 19° 30′; breadth at end about $3\frac{1}{2}°$. It extended to a star No. 60 of *Hydra* (in *Uranometria Argentina*).

November 9, $12^h\ 30^m$.—Perhaps faint traces of a dark streak. Tail wider; two stars, 141 and 160 in *Hydra*, neatly enclosed

it; *i.e.* were lying one on each edge of the tail. Head made a pretty combination with two stars, 174 and 175 *Hydræ*. Nucleus as seen in telescope a dim woolly ball, no bright line as before. Position at $13^h\ 7^m$: R.A. $9^h\ 40^m\ 53^s$; S. Dec. 22° 44′; length of tail, 20°; breadth at end, $3\frac{1}{2}°$.

November 12.—Light very delicately graduated from head to tail. Head is now about equal to a 5th magnitude star in brightness. The head reminded me of the bright nebula near the Nubecula Minor. Position at $13^h\ 22^m$: R.A. $9^h\ 34^m\ 47^s$; S. Dec. 23° 45′; length of tail, 19°; breadth at end about 5°.

November 20, $14^h\ 30^m$ to $17^h\ 40^m$.—Comet generally brighter, I think, than when last seen, close to a 5th magnitude star, to which it was superior in brightness. Observations very much hindered by clouds. I compared its place with *θ Pyxis*. Position at $15^h\ 23^m$: R.A. $9^h\ 16^m\ 54^s$; S. Dec. 26° 0′.

November 30, 15^h.—Comet almost in the zenith. Light much obscured by moonlight, yet there was a long tail visible, and head was as bright as a 5th magnitude star. In telescope head was woolly, and tail barely discernible. Comparing it with 42 *Pyxis* I got position at $15^h\ 21^m$: R.A. $8^h\ 50^m\ 8^s$; S. Dec. 28° 42′; length of tail, 15°; breadth at end, $4\frac{1}{2}°$.

December 8, 11^h.—Very much fainter. Head looks like a diffused nebula in telescope, with no sign of nucleus. Tail faint, but wide; traced to about R.A. $8^h\ 0^m$; S. Dec. 20°; length 8°. No star near for comparison. I estimate position roughly at $11^h\ 15^m$: R.A. $8^h\ 28^m$; S. Dec. 29° 30′.

December 10, $10^h\ 15^m$.—Very clear sky. Tail straight, and extending for some 12°. Position roughly estimated at $10^h\ 15^m$: R.A. $8^h\ 22^m$; S. Dec. 30° 21′.

December 15, $14^h\ 30^m$.—Comet near the zenith. At times it was very clear during observations, and the tail could still be seen of very considerable length and breadth. Position: R.A. $8^h\ 4^m$; S. Dec. 30° 40′; length of tail about 15°; width at end 5°. Tail faded away very gradually, so that it is difficult to say exactly where it terminated. It was directed towards a point: R.A. $7^h\ 40^m$; S. Dec. 20° 0′.

December 22, $12^h\ 30^m$.—Comet very dim owing to bright moonlight. No tail could be seen. The following position was roughly estimated: R.A. $7^h\ 37^m$; S. Dec. 30° 0′.

Observations of Comet a, 1883 (*Brooks-Swift*), *made at the Cambridge Observatory, with the Northumberland Equatorial and Square-bar Micrometer.*

(*Communicated by Prof. J. C. Adams, M.A., F.R.S.*)

Green. M. T.	Aberration Time.	App. R.A.	Parallax.	App. Decl.	Parallax.	Comp. Star.	No. of Comp.
1883.		h m s	s	° ′ ″	″		
Mar. 3·29997	−·00666	0 12 23·663	+0·371	+32 0 47·27	+4·97	*a*	5
3·32017	−·00666	0 12 34·891	0·371	32 0 48·25	5·29	*a*	5
5·29434	−·00670	0 31 27·105	0·366	31 59 49·00	4·74	*b*	5
5·30642	−·00670	0 31 33·276	0·369	31 59 50·75	4·93	*c*	5
8·29263	−·00679	0 59 18·430	0·353	31 35 58·33	4·50	*d*	5
,,	,,	17·352	,,	56·86	,,	*e*	5
8·30639	−·00679	0 59 26·352	0·359	31 35 49·36	4·71	*d*	5
,,	,,	25·475	,,	48·65	,,	*e*	5
9·30323	−·00684	1 8 23·237	0·354	31 22 22·57	4·60	*f*	5
,,	,,	23·380	,,	26·03	,,	*g*	5
9·35984	−·00684	1 8 53·118	0·355	31 21 35·37	5·44	*f*	5
,,	,,	53·183	,,	37·65	,,	*g*	5
15·31233	−·00719	1 58 8·749	0·325	29 18 10·06	4·37	*h*	5
,,	,,	*i*−2 2·784	,,	*i*−0 10·32	,,	*i*	5
15·33099	−·00719	1 58 17 470	0·332	29 17 39·65	4·63	*h*	5
,,	,,	*i*−1 54·187	,,	*i*−0 39·91	,,	*i*	5
30·35196	−·00861	*j*+1 22·438	0·261	*j*−14 55·13	4·17	*j*	5
,,	,,	3 28 52·526	,,	21 52 56·93	,,	*k*	5
30·37434	−·00861	*j*+1 28·132	0·263	*j*−15 36·04	4·36	*j*	5
,,	,,	3 28 58·244	,,	21 52 13·08	,,	*k*	5
31·33700	−·00871	3 33 24·781	0·252	21 23 47·29	4·01	*l*	5
,,	,,	24·968	,,	47·13	,,	*m*	5
31·35294	−·00872	3 33 29·308	0·257	21 23 20·72	4·14	*l*	5
,,	,,	29·640	,,	15·27	,,	*m*	5
Apr. 2·35507	−·00894	3 42 16·827	0·249	20 24 55·87	4·08	*n*	5
,,	,,	*o*+1 32·089	,,	*o*+8 59·35	,,	*o*	5
2·36968	−·00894	3 42 20·748	0·251	20 24 27·76	4·20	*p*	5
,,	,,	*o*+1 36·301	,,	*o*+8 35·11	,,	*o*	5
7·34322	−·00959	*q*−1 21·347	0·226	*q*−3 41·35	3·80	*q*	5
,,		4 2 5·764	,,	18 6 11·58	,,	*r*	5

Assumed Mean Places for 1883·0 *of Stars compared with Comet* a, 1883 (*Brooks-Swift*), *with Reductions to Apparent Places for the night of Observation.*

	Right Ascension.	Reduc.	Declination.	Reduc.	Authority.
	h m s	s	° ′ ″	″	
a	0 14 38·573	+0·218	+32 15 43·87	+5·43	Micrometer comparison with Bessel 0h 348. *a* is Arg. 32° 45.
b	0 29 54·047	0·271	32 2 27·16	5·20	Bessel 0h 725.
c	0 33 13·856	0·284	31 59 11·95	5·21	„ 822.
d	0 59 45·265	0·378	31 33 22·10	4·78	„ 1464.
e	1 1 32·073	0·386	31 23 18·48	4·75	„ 1505.
f	1 6 24·296	0·401	31 27 15·46	4·61	Arg. 31° 197 meridian observation.
g	1 8 12·306	0·407	31 24 3·05	4·60	Bessel 1h 109.
h	1 57 13·867	0·569	29 20 30·13	3·07	Cambridge Zones, 1879, Dec. 22.
i	2 0 9·2	0·581	29 19 43	+3·04	Arg. 29° 359.
j	3 27 29·5	0·746	22 7 54	−1·76	„ 22° 509.
k	3 30 32·978	0·757	21 57 53·20	−1·90	Bessel 3h 620, 621.
l	3 31 7·060	0·743	21 19 23·76	−2·13	„ 631.
m	3 32 23·463	0·763	21 27 40·18	−2·13	„ 655.
n	3 37 56·876	0·740	20 22 44·20	−2·67	„ 809.
o	3 40 43·8	0·749	20 15 57	−2·77	
p	3 40 39·657	0·749	20 13 2·26	−2·78	Arg. 20° 633, meridian observation.
q	4 3 25·2	0·765	18 8 47	−4·22	Arg. 18° 493.
r	4 3 55·909	0·765	18 7 0·90	−4·22	Bessel 4h 10, 11.

The above observations appear to show that the difference of R.A. of stars *d* and *e*, as deduced from Bessel, is 1s in error.

The following parabolic elements have been calculated by Mr. Graham from the observations of March 3, 9, and 15 :—

T = 1883, Feb. 18·952355, G.M.T.

$\pi = 29^\circ\ 2'\ 45''.32$
$\Omega = 278\ 8\ 15.57$ } Mean Equinox, 1883·0.
$i = 78\ 3\ 26.33$

$\log q = 9.8808596$. Motion direct.

The middle place is represented thus :—

Obs. − Calc. $\quad \Delta L \cos b = -0''.80. \quad \Delta b = +1''.10$;

where L and *b* indicate the geocentric longitude and latitude.

Observations of Comet a, 1883, *made at the Royal Observatory, Greenwich.*

(*Communicated by the Astronomer Royal.*)

The observations were made with the East or Sheepshanks Equatorial, by taking transits over two cross wires at right angles to each other, and inclined 45° to the parallel of declination.

Green. Mean Solar Time.	Obs.	δ − ✶ R.A.	δ − ✶ N.P.D.	No. of Comp.	Apparent R.A.	Apparent N.P.D.	Star.
d h m		m s	′ ″		h m s	° ′ ″	
Mar. 9 7 55	T.	+2 11·33	+ 5 28·5	3			*a*
		+0 23·50	+ 2 18·7	3	1 8 36·03	58 38 8·3	*b*
8 45		−2 1·67	−14 2·2	3	1 8 53·60	58 38 14·0	*c*
9 0		+2 35·83	+ 6 4·8	3			*a*
		+0 47·33	+ 2 46·5	3	1 8 59·86	58 38 36·1	*b*
9 8		−1 52·00	−13 40·8	1	1 9 3·27	58 38 35·4	*c*

Mean Places of the Comparison Stars.

Star.	Star's Name.	R.A. 1883·0.	N.P.D. 1883·0.	Authority.
		h m s	° ′ ″	
a	Anonymous			
b	W. B. (2) I.−109	1 8 12·12	58 35 54·2	W. B. (2)
c	W. B. (2) I.−175–6	1 10 54·85	58 52 20·7	W. B. (2)

Note.—Comet faint and diffused.

The observations are not corrected for refraction or parallax. The comet was also observed on March 12 and 27, but the comparison stars cannot be identified.

Royal Observatory, Greenwich:
1883, *April* 13.

Spectroscopic Observations of Comet a, 1883 (*Brooks-Swift*).

By Dr. N. de Konkoly.

The observations were made with the large Refractor (10-inch aperture), by Merz, on March 3, at 8^h mean time, at an altitude of 17°.

The spectroscope used was similar to that I constructed for the Brussels Observatory, with a colorimeter, made for me kindly by my friend E. de Gothard, owner of a beautiful Observatory at Herény, near Steinamanger (Hungary), with an excellent direct vision prism, by Dr. Hugo Schröder.

I saw in the spectrum only three bright bands, and with a widely-opened slit a very faint continuous spectrum, of which

I estimated the ends to be respectively 565·0 and 470·0 mm. wave-length.

I estimated the intensity of the bands at 0·6, 1·0, and 0·3. The faintest was near the violet end of the spectrum. The positions of the bands were

I.	559·9 mm. (of wave length).	
II.	515·6	„ „
III.	470·2	„ „

The lines correspond very well with the bands in the hydrocarbon spectrum.

The position of the bands given is that of their maximum intensity.

OGyalla Observatory:
1883, *March* 12.

Note on the Eclipse of Jupiter's 4th Satellite on April 4.

By Capt. W. Noble.

The satellite did not even begin to fade out until some minutes after the predicted time, and did so very gradually indeed, flashing up at intervals in a way which I could hardly persuade myself was wholly referable to atmospheric undulation. I did not finally lose sight of it until $9^h\ 58^m\ 18^s$ Local Mean Time $= 9^h\ 58^m\ 0^s$ G.M.T. (the longitude of my Observatory being 17·8 seconds east of Greenwich). Its disappearance was announced in the *Nautical Almanac* as occurring at $9^h\ 33^m\ 54^s$ G.M.T.; a prediction thus shown to be $24^m\ 6^s$ wrong. Of course a larger telescope than mine would have held the satellite still longer in view. I would venture to suggest that the "explanation" on pp. 501 and 502 of the *Nautical Almanac* should be supplemented by a caution as to the utter untrustworthiness of such predictions as the one on which I am commenting. I know how imperfect Damoiseau's Tables are. All I contend for is that the ephemerides calculated from them should not be given to the public without a single word of warning that they are not to be depended upon within four-and-twenty minutes. The instrument employed was my 4·2-in. Ross Equatorial, with a power of 154.

Forest Lodge, Maresfield, Uckfield:
1883, *April* 12.

[Note.—It was explained to the meeting by Mr. Marth, that at the beginning or end of a series of Eclipses of the Fourth Satellite the times of duration were very variable, and that the difference of 24^m did not imply any great error in the Tables, because the slightest difference in latitude made an enormous difference in the length of the chord within the shadow-cone described by the satellite.—Ed.]

Observations made at the U.S. Naval Observatory, Washington.
By Professor Asaph Hall.

(*Communicated by Vice-Admiral Rowan, Supt.*)

Conjunctions of the Satellites of Saturn.

Satellite.	Date.	Wash. M.T.	Position.	Wt.	Obs.
Mimas ...	1882, Oct. 5	13 11·4	S. foll.	3	H.
	Nov. 8	8 44·2	S.	2	F.
Enceladus ...	Oct. 5	13 6·4	S.	3	H.
	14	10 39·8	N.	1	H.
	Nov. 22	10 36·8	S.	3	F.
Tethys ...	Oct. 18	12 28·2	S.	3	H.
	20	9 41·6	N.	2	H.
	Nov. 21	10 40·8	S.	2	F.
	22	10 9·3	N.	3	F.
	23	9 54·8	S.	3	F.
	24	9 23·8	N.	3	F.
	Dec. 26	9 28·2	N.	2	H.
	28	6 41·8	N.	3	H.
	1883, Jan. 12	9 11·0	N.	3	H.
Dione ...	1882, Dec. 16	8 37·8	N.	2	F.
	1883, Jan. 11	8 36·4	S.	2	H
	22	7 20·4	S.	2	H.
Rhea ...	1882, Nov. 21	10 41·3	S.	2	F.
	1883, Jan. 12	9 25·5	N.	3	H.

The Companion of Sirius.

Date.	Sid. Time.	*p.*	*s.*	Wt.	Obs.
	h	°	″		
1883, Feb. 23	6·5	44·0	10·02	2	F.
27	6·8	40·6	10·01	3	F.
28	7·1	40·9	9·39	3	F.
Mar. 1	8·0	41·8	9·84	3	F.
7	6·6	40·4	9·53	3	F.
8	6·6	42·0	9·75	2	F.
9	7·2	40·3	9·74	2	F.
14	6·2	39·8	9·39	2	H.
16	6·2	38·7	9·12	3	H.
17	6·3	38·3	9·32	2	H.
18	6·2	39·5	9·19	3	H.
20	6·5	39·4	9·31	2	H.
21	6·6	38·9	9·23	2	H.

Mean Results.

	p.	*s.*	
	°	″	
1883·170	41·43	9·754	E. Frisby.
1883·211	39·10	9·260	A. Hall.

Remarks.—The observed conjunctions of the satellites of *Saturn* are with the minor axis of the ring. The wire of the micrometer was set parallel to this axis by means of the angle given in the *American Ephemeris*, p. 479. The conjunction of *Mimas* on October 5 was with the following end of the principal division of the ring.

Note on the Great Comet (b) 1882.

An examination of the nucleus of this comet in the latter part of February 1883, showed two bright points of condensation, with two fainter points, one on each side of the bright ones. These four points were in a right line, and the general appearance was nearly like that given by the drawing of Mr. Prince, *Monthly Notices*, January 1883, p. 85. The following of the two bright points seemed to be the brighter, and calling this point *a*, the other bright point *b*, the preceding faint point *c*, and the following faint point *x*, the following measures were made, the origin being at *a*; *c* and *x* were very faint:—

1883.	Sid. T.	*p.*	*a* to *b*	*b* to *c*	*a* to *x*	Obs.
	h	°	″	″		
Feb. 26	6·4	257·2	34·50	47·14	—	H.
27	6·0	258·8	34·58	48·99	22·27	H.

On the Visibility of the Dark Side of Venus.

By Prof. C. V. Zenger.

It is known that the first observation of the Moon's ashy light was made by Michel Maestlin in 1520, but the first observation has been also ascribed to Leonardo da Vinci, who died in that year.

A similar appearance was detected by Riccioli on January 9, 1643, on the planet *Venus* with the then discovered Galilean tube. He says: "Erat planeta Solem versus rubicunda in medio flavescens et in parte a Sole aversa cæruleo-viridis, sed illa varietas a vitro tubi probabiliter fuit." The following passage seems to indicate a very fine state of the weather: "Semiannulus lucidus, quo a tergo coronabatur, erat forte a *Jove* et *Saturno* illam illustrantibus, utpote orientalioribus" (*Almagestum novum*). It is obvious that the reddish hue may have been partly due to the chromatic aberration of the tube, but the

greenish-blue tint of the dark side was later observed by Harding and Engelmann. Hahne saw it tinted greyish-brown, and Harding also greyish-red.

I could never see the whole dark side myself, and was much inclined to regard these observations as an involuntary illusion, imagination inducing us to complete the partly visible disk, whose circumference is obviously indicated by the part visible on the crescent. Yet there are some observations opposed to this view, by Kirch, on June 7, 1721, and March 8, 1726, when he observed that the dark side seemed a circle of less diameter than that of the crescent. He mentions that he once more saw the entire dark side of *Venus* on October 20, 1759, at noon, although the southern declination of 21° 50′ greatly interfered with a good definition.

Harding made the most trustworthy and accurate observations of any observer, using a large reflector of 10 feet focal length, and a power of 84 diameters. He also saw *Venus* beautifully on January 28, 1806, with the whole aperture of the reflector, and the cusp projecting over the dark side. Another time, February 20, 1806, at $6^h\ 12^m$, he saw again the entire disk of *Venus*, of reddish-grey hue. Schröter saw, on February 14, 7^h, *Venus* for the first time entire, with the reflector of 20 inches aperture, and 27 feet focal length, in a very feeble light, the hue of which he does not describe.

Gruithuisen, on June 7, 16^h (21 days after conjunction), likewise saw the phenomenon well, though at low altitude.

Finally, Engelmann, on April 20, 1865, Banks, Green, Noble, and Arcimis, 1877, Noble, Mills and Webb, 1878, have seen the phenomenon; but Lassell could not see a trace of the dark side, though he observed with a powerful instrument. This may be perhaps due to the very far advanced diminution of the crescent, which he describes as a hair of light rather than a crescent. The feeble light of the dark side must then vanish, *Venus* being so near to the blazing sunlight; and indeed he says that some days afterwards solar light still invaded the tube and seriously disturbed accurate vision. Though the observers agree as to a greyish colour of the dark side, yet they differ considerably as to the additional tint, calling it reddish, brownish, greenish, ashy, &c. It seemed to me desirable to observe *Venus* after conjunction for the purpose of deciding upon the visibility of the dark side, and the ring, observed solely by Riccioli.

I prepared in the first instance to correct the 4-inch Equatorial I intended to use for the observation as far as possible for all chromatic aberration. A fine specimen of a Barlow lens by Browning was placed in the tube; the objective being slightly under corrected, it was possible to get rid of all traces of colour by placing the Barlow lens in the right position to the objective lens. Trying it on the planet itself till all traces of colour disappeared seemed to me the safest and most expeditious means. It was only in entire darkness that a trace of reddish colour

could be detected on the outer edge of the crescent. Aplanatism was nearly perfect.

Prepared in this manner I waited in the discomforting season of 1882 in vain for a suitable opportunity. Finally, in January 1883, a couple of most splendid mornings favoured us in Prague, accompanied by heavy frost, and no trace of cloud visible all over the horizon. It was possible to set to work at once, and it was first possible on January 8, and then on January 9, between 18^h and 20^h, to see *Venus* beautifully with powers of 60 and 120 diameters, projected on a cloudless dark blue sky.

I did not expect to see such a ravishing spectacle as was disclosed at once to my sight on the 8th inst., $18^h\ 45^m$; *Venus* not only was visible as a whole disk, but there was an amount of detail in the aspect that I believe to be sufficient proof of the reality of the phenomenon. The crescent overlapped nearly $8''$ (at least) the adjacent border of the unilluminated part of the disk, the terminator was all along its length serrated, and the reddish-yellow light of dawn invaded the dark disk for at least $\frac{1}{6}$th of the whole diameter, shading down and fading away on the border of the dark part. This was splendidly visible even to the unaccustomed eye of entirely uninstructed persons, to whom I did not mention the appearance of the planet, but who had seen the Moon as a crescent. They at once recognised the whole disk, well defined and projected on the dark blue sky.

But the most important feature of the observation was the ring, that I could detect all round the disk (dark part and crescent), of brownish-red colour, more pronounced on the illuminated side than on the dark part of the limb, but of a peculiar coppery hue, the close resemblance of which to the coppery hue the Moon's disk assumes when totally eclipsed was very striking.

As it is certain this hue is due to the absorption of the more refractive part of terrestrial light in the atmosphere of the Earth, it seems to me very probable that the reddish-brown ring of light round *Venus* is due to refracted light in the atmosphere, and to the reflected light from the clouds in the planet's atmosphere. Now, supposing *Venus's* atmosphere to be similar to our own, a similar hue by selective absorption must be produced, and the cause of resemblance of the colour of the eclipsed Moon and of the ring round *Venus* is then obvious.

At $18^h\ 56^m$, January 9, I could discern on the southern horn a most striking feature, very near to the end of the southern cusp, which was more sharply edged than the northern one. It was not only indented as the other parts of the terminator, but there was clearly seen an object of perhaps $1''$ of diameter standing out boldly in the midst of a more or less greyish-tinted elliptical zone, forming a deep indentation of the end of the terminator. The shading round it was very well visible, and it seemed to me a high mountain of oblong form, piercing with its refulgent light through the surrounding valleys still immersed in half shadow or in full shadow. This is the second time that

I have observed such an isolated brilliant point of appreciable diameter on the southern cusp of *Venus* (*Monthly Notices*, 1877, p. 460).

The dark part of *Venus* seemed to me not bluish-grey, nor greenish-grey; but from the terminator, where the dawning of sunlight produced a pronounced reddish-yellow zone, it faded away to a decidedly reddish-grey tint, bordered by the more luminous reddish-brown of the extremely narrow ring of perhaps atmospheric origin.

We can scarcely imagine a more probable supposition of its nature than a similarity of absorbing power of the terrestrial atmosphere and of *Venus's* atmosphere producing the same coppery hue as is visible on the eclipsed Moon's surface, illuminated by the reflected rays of sunlight from the Earth's surface, and deprived by selective absorption of the more refrangible rays.

Prague,
1883, *Jan.* 10.

Addition.

It happened that on the 11th and 12th of January, the Moon was a small crescent, and the atmosphere of extraordinary steadiness and clearness, showing the Moon's ashy light with most remarkable clearness, the mountains being visible as I had never seen them before in the dark side.

It was after a most trying period of wet and cloudy weather that the sky began to clear up from the 7th, the 8th being again cloudy, but from the 9th to the 13th there was a singular clearness all over the heavens, there being a deep blue from morning to evening. It was this clearness that obviously increased the visibility of the dark side of *Venus* as well as of the Moon. But it shows also clearly to me the parallelism of the phenomena of visibility of the dark sides of *Venus* and of the Moon. On the 12th, and after, the dark side of the Moon was no longer visible to the naked eye, and it was the same with the crescent of *Venus*. It is only possible to see its dark side to advantage, if:—

1. The atmospheric conditions are exceptionally fine;

2. If a certain part of the disk is only illuminated, and *Venus* is at her greatest luminosity, or near to it, as has just been the case.

There is a certain advantage if the luminous part is not too great, and if it is in its greatest luminosity; the feeble light of the dark side is then likewise at its utmost visibility.

Venus' dark side is therefore visible not by fluorescence of the sea, nor by auroral light, but simply by the same causes as those that make the Moon's dark side visible.